06-121

DOCKET FILE COPY ORIGINAL

November 7, 2007

FILED/ACCEPTED
NOV - 7 2007
Federal Communications Commission
Office of the Secretary

Ms. Marlene Dortch
Secretary
Federal Communications Commission
445 12th St SW
Washington, DC 20554

Re. MB Docket No. 06-121

Dear Ms. Dortch:

Please enter the enclosed documents into the official record for the above-referenced proceeding. These are peer reviews conducted by outside experts at the request of the Commission and concern twenty-two economic studies submitted by parties to the Federal Communication Commission's Media Ownership Quadrennial Review proceeding. The studies addressed by the peer reviews are available in the Commission's Electronic Comment Filing System, and links to the studies are available on the Commission's web site, at http://www.fcc.gov/mb/peer_review/reviews.html.

The attached peer reviews are being made a part of the record pursuant to the Office of Management and Budget Final Information Quality Bulletin for Peer Review, 70 Fed. Reg. 2664 (2005). The findings and conclusions in the peer reviews as well as those in the studies to which they refer are those of the authors and do not necessarily represent the views of the Commission.

Sincerely,

Monica Shah Desai

Monica Shah Desai
Chief, Media Bureau

No. of Copies rec'd 0+1
List A B C D E

Referee report by Stefano DellaVigna, UC Berkeley

Newspaper/Television Cross-Ownership and Local News and Public Affairs Programming on Television: An Empirical Analysis by Michael Yan

This paper analyses empirically the impact of newspaper/television cross-ownership on the programming of local television stations. In particular, the paper is focused on whether stations that are cross-owned provide more local news and more public affairs programming.

The evaluation is done comparing the programming of cross-owned and non-cross-owned stations on 14 days in 2003. A first comparison of the programming of the two types of stations suggests that cross-owned stations indeed provide more local news and more public affairs programming. Since this difference may be due to other factors such as differences across stations in geographical location, size of the audience, etc, the authors estimate regressions that control for some of these differences.

The regression results imply that, even controlling for other factors, cross-owned stations are significantly more likely to offer local news programming. However, conditional on offering local news programming, the amount of public programming offered is not significantly different from cross-owned and non-cross-owned stations. Further, there is no statistically significant relationship between cross-ownership and public affairs programming.

The authors summarize the results as saying that there is no evidence that cross-owned stations provide more local news and public affairs programming.

The study is based on a fairly simple methodology that compares the programming or the two types of stations, controlling for confounding factors. The methodology is, per se, appropriate, with some caveats detailed below.

The summary conclusion that the authors draw from the study, however, is not warranted given the findings. The first finding in the paper is that, even after controlling for a number of other factors, such as big-4 affiliate, whether the owner is local, whether there is a national reach, etc., the presence of cross-ownership significantly *increases* the probability that a station will broadcast local news. In fact, the variable "cross-ownership" is statistically the most significant predictor among the 16 variables in the regression. This finding does not fit with the conclusion of the authors.

The second finding is that, *conditional on offering local news*, the quantity of local news offered by the station is instead not significantly affected by the presence of cross-ownership. It is true that, in this case, as the authors say, there is no significant relationship for a positive effect of cross-ownership on the quantity of news provided. However, the sign of the coefficient on cross-ownership is still positive and quite sizeable: the size of the coefficient for example is about half the size of the coefficient on the "VHF Status" variable, which is significant. I would summarize this evidence as saying that there is weak (not statistically significant) evidence that, even conditional on availability of local news, there may be some effect of cross-ownership on the quantity of local news broadcast.

Taking the two pieces of evidence together, I would summarize them as follows. There is evidence that cross-ownership is associated with a large and statistically significant increase in the availability of some degree of local news programming; the evidence is instead more mixed on whether the quantity of programming, conditional on any programming at all, is related to cross-ownership. Below, however, I discuss a reason, endogeneity, that leads me to think that this positive correlation is not likely to reflect a causal relation.

As for the second set of findings, on local public affairs programming, these findings are more tentative because the number of minutes devoted to this programming across stations is much lower. Hence, it takes a larger number of observations to test conclusively whether a variable, such as cross-ownership, has a significant effect or not on this programming. This being said, the overall evidence does not suggest a strong relationship between cross-ownership and local public affairs programming. This is indeed as the authors say. Cross-ownership somewhat reduces the probability of any public affairs programming (though not significantly so) and somewhat increases the quantity of such programming (though again not significantly so). In my view, one would need more data to conclude on this second relationship, given the small space devoted to this programming on the stations.

One additional key issue comes into play in this evaluation. Due to endogeneity, the methodology used by the authors is not certain to provide a correct measure of the impact of cross-ownership. Cross-ownership is not randomly assigned across stations. Ownership decisions are made by profit-maximizing companies. The stations that are bought over by a company that also owns newspapers are likely to be so when the television stations are natural outlets for the content of the newspapers. The televisions that are cross-owned, therefore, may provide more local news programming, but these stations would have provided more programming even were they not taken over.

To keep things simple, consider Area A where there is very little demand for local news, and the television stations do not provide much local news. In Area B, instead, there is more demand for local news and the televisions indeed provide such news. Now assume that we allow cross-ownership. The newspaper owners are more likely to buy stations in area B rather than in area A, since doing so provides more of an outlet for the news already gathered by the newspaper, allowing for cost-saving. However, the station that is now cross-owned would have provided more news even *absent* the cross-ownership! Hence, it is not the cross-ownership that is causing the local news programming, but rather the opposite.

This kind of endogeneity problems implies that one should be very careful with conclusions from cross-sectional studies such as the one here examined. In general, the endogeneity would bias upward the estimates of the impact of cross-ownership on the provision of local news, since the stations that already were providing local news are more likely to be a target for cross-ownership. Hence, the limited evidence in this paper that cross-owned stations provide more local news may be due to endogeneity, rather than being a causal relationship. Obviously, this completely changes the interpretation of the findings.

Summary: I have criticized the conclusion of this paper that there is no evidence of an effect of cross-ownership on provision of local news programming. Following the simple methodology in this paper, one finds instead some evidence.

However, I *agree* with the authors that there is no strong or convincing evidence that cross-ownership increases the availability of local news programming, since the simple methodology used in this paper is likely to give biased estimates.

The evidence of a positive correlation between cross-ownership and provision of local news is likely to be due to endogeneity of cross-ownership. I certainly would not take the result of this study to be that we can be sure that cross-ownership increases local news programming. In my view, the jury is still out there. The existing evidence does not prove to any reasonable degree the existence of benefits to cross-ownership (nor does it prove the contrary).

To provide evidence that is more indicative of such relationship, it would be useful to observe the amount of programming *before* and *after* a station become cross-owned. Doing such an over-time study would allow, at least to a first approximation, to control for the endogeneity of cross-ownership as detailed above.

On a more detailed note, the authors should report the number of observations, the mean for the dependent variable, and the procedure used to derive standard errors in the notes to the Tables (are the s.e.s robust? Are they clustered by station as they should?)

Also, the authors should say how the 14 days in which the programming was measured were chosen. Where the 14 days randomly chosen? This would seem important for the purposes of the evaluation.

Statement of Charles Romeo concerning studies submitted in MB Docket 06-121, and FCC MN Docket 06-121

September 18, 2007

Background and Qualifications

1. I am a Research Economist in the Antitrust Division at the US Department of Justice.* I was previously an Assistant Professor of Economics at Rutgers University, in New Brunswick, NJ. I earned my Ph.D. in Economics from Duke University in 1989.

2. My research specialties are econometrics and applied microeconomics. I have published 11 academic papers in these areas, and have received one National Science Foundation grant in support of my research.

Conclusions

1. I have been asked by the FCC to review three submissions by Professor Jerry Hausman. Two of the submissions are themselves reviews of two other submissions. His reviews were submitted on January 16, 2007. The first submission reviews, *Newspaper/Television Cross-Ownership and Local News and Public Affairs Programming on Television Stations: An Empirical Analysis* by Professor Michael Yan, which was submitted to the FCC by the Donald McGannon Communications Research Center on October 23, 2006. The second submission reviews, *Consolidation and Conglomeration Diminish Viewpoint Diversity and Do Not Promote the Public Interest: New Evidence (Study 16)*, by Mark Cooper and S. Derek Turner. This was also submitted on October 23, 2006. The third submission by Professor Hausman is Exhibit 2, in *Comments of Clear Channel Communications, Inc.* Submitted on October 23, 2006.

---

*These reviews were done in my personal capacity and the views expressed are my own professional judgements and are not purported to reflect those of the United States Department of Justice.

The Yan and Cooper and Turner submissions

2. The Yan and Cooper and Turner submissions focus on newspaper/television cross-ownership. Professor Hausman limits attention in his reviews to the econometric analysis in the Yan and Cooper and Turner submissions, so I will do likewise. Moreover, given that I have been tasked to review reviews, I will focus mainly on Professor Hausman's comments, but I do find it necessary to also reference the original studies.

3. I have come to the following conclusions regarding these submissions:

Comments on Professor Hausman's Review of the Yan Study

- I agree with Professor Hausman's three main points regarding the Yan submission: the paper lacks a sufficient description of the data, the main outcomes models he estimates contain an identification error, and Professor Yan's results in fact show the opposite of what he concludes.
- The only additional conclusions that I have reached, are ones that I feel are missing from Professor Hausman's comments. Professor Hausman discusses the econometric results, but does not discuss the economic mechanisms underlying them. I make two points.
- My first point has to do with the implications of the cross ownership coefficient in the selection model in Table 4 of the Yan paper. This coefficient is large and highly statistically significant and indicates that the provision of news programming is much higher among cross-owned stations. Professor Hausman points this out in part by noting that even if minutes of local news broadcasting are equal across cross-owned and non cross-owned stations, total minutes of news programming, which is the product minutes

and the probability that a station broadcasts local news is greater among cross-owned stations. That is,

$$TMIN_{CO} = P_{CO}*m >> P_{NCO}*m = TMIN_{NCO}$$

where $TMIN_{CO}$ is the total minutes of local news broadcast by a cross-owned station, P_{CO} is the probability that a cross-owned station broadcasts local news, $TMIN_{NCO}$ and P_{NCO} are similarly defined for non cross-owned stations. Minutes of news broadcasting, m, is assumed to be common across cross-owned and non cross-owned stations. Professor Yan's results indicate that $P_{CO} >> P_{NCO}$. Hence, even if m is equal across cross-ownership types $TMIN_{CO} >> TMIN_{NCO}$.

The economic point to be emphasized is that $P_{CO} >> P_{NCO}$ suggests that synergies from cross-ownership impact local news broadcasting mainly through the provision decision. Being able to draw on a staff of local news reporters likely lower costs of providing news and therefore increases the probability it gets provided.

- My second point relates to the cross-ownership coefficients in the local news and public affairs programming outcome regressions in Tables 4 and 5. As Professor Hausman points out, Professor Yan's results suggest that minutes of local news broadcasting and minutes of public affairs programming may actually be higher for cross-owned stations.[1] This in turn suggests that synergies may operate through the length of local news and public affairs programming decisions as well as through the provision decision. This will be true if being able to draw on a local reportorial staff lowers the costs of these types of

[1]Professor Yan estimates the cross-ownership coefficient to be positive but statistically insignificant in both his local news and public affairs minutes regressions. However, problems in his econometric work up, a censoring issue in the data, and small sample sizes may be causing these estimates to be insignificant.

programming relative to others and thereby increases the profit maximizing quantities of both local news and public affairs programming.

Comments on Professor Hausman's Review of the of the Cooper and Turner Study

- I agree with Professor Hausman's main conclusion that "CT's results provide evidence that cross-ownership leads to more minutes of both local news and local public affairs." To Professor Hausman's conclusions, I add one technical conclusion. I disagree with CT's use of a Tobit model in estimating the regression for minutes of Public Affairs programming. Tobit models are employed when the dependent variable for regression is censored for a subset of sample observations. Censoring typically occurs when a survey instrument or a policy bounds a variable above or below a specific point: wage rates being censored at the minimum wage is a leading example. CT use a Tobit model because many of the stations in their dataset produce zero minutes of public affairs programming. But treating public affairs minutes as censored at zero implies both that negative minutes of programming are possible and that stations desire to air a negative quantity of Public Affairs minutes. If a market existed where one station could buy the minutes that another station uses for Public Affairs programming and put it to a different use, then this could be considered as airing negative minutes. Since this market does not exist, CT's Tobit based results, which Professor Hausman assesses as providing evidence that cross-ownership leads to more minutes of public affairs programming, are unreliable at best.

Comments on Statement of Professor Jerry A. Hausman, Exhibit 2 in Clear Channel submission entitled *Comments of Clear Channel Communications, Inc.*

1. Professor Hausman discusses three topics in this submission: consolidation and format diversity, consolidation and advertising prices, and the volatility of radio station market shares. He presents new empirical results for the first and third topics and limits study of the second topic to a discussion of the *DOJ/FTC Merger Guidelines* and two published papers. Given this, I will limit my comments to a discussion of his econometric analysis of topics one and three.

2. Consolidation and Format Diversity: To assess the impact of ownership consolidation resulting from the 1996 Telecom Act on format diversity Professor Hausman uses an approach developed by Steven Berry and Joel Waldfogel[2] and extends their work by incorporating additional years of data that have accrued since the Berry and Waldfogel paper was published in 2001. The model Professor Hausman estimates uses number of formats in each local market as the dependent variable, and number of owners, population and market and time fixed effects as independent variables. His sample contains data from 243 Arbitron markets in each of four years: 1993, 1997, 2001, and 2006. He follows Berry and Waldfogel in controlling for endogeneity by instrumenting for number of owners using a variable based on the policy bands created by the 1996 Telecom Act. Professor Hausman's results show that a decrease in the number of owners has increased the number of formats. He cites this as evidence that format variety has increased with ownership concentration. I have reached the following conclusions regarding this result.

- The method employed by Professor Hausman is sound and the result is consistent with a similar result using only 1993 and 1997 data presented by Berry and Waldfogel.

---

[2] *Do Mergers Increase Product Varitey? Evidence from Radio Broadcasting*, Quarterly Journal of Economics, (2001), V. 116(3), 1009-1025.

However, there are caveats to be noted.

- First, Professor Hausman presents results from only a single regression. Berry and Waldfogel presented results that examined the impact of ownership concentration on the number of formats, the number of stations, and the number of formats/station. These regressions might tell different stories about the impact of ownership concentration. Second, Berry and Waldfogel, were concerned that their policy band variable might not be a valid instrument and so they conducted robustness checks using different regression formulations and different instrument sets. Third, as is stated in the Clear Channel submission, the number of radio stations has increased from 12,140 in 1996 to more than 13,700 today, an increase of 12.8 percent.[3] Hence, it is possible that in markets with high station growth, the number of formats may have increased even if ownership consolidation reduces format variety. To the extent that population variables and fixed effects do not serve as adequate proxies for the growth in the number of stations, this effect is uncontrolled for in Professor Hausman's regression and may bias the ownership effect. In particular, if station growth was highest in markets that consolidated most, this effect would become stronger. Showing that Professor Hausman's result holds in a regression using formats/station as the dependent variable would alleviate this concern.

3. Volatility of Market Shares: Referencing the *FTC/DOJ Merger Guidelines*, Professor Hausman articulates that for the purposes of evaluating the competitive significance of mergers, market shares should represent future competitive significance. In the case of radio, he argues, that all firms have an equal likelihood of securing future sales and so radio firms should be assigned equal

[3] *Comments of Clear Channel Communications Inc.*, page 7.

market shares. To support this conclusion, he references a paper by DOJ economist Gregory Werden[4] that argues for "one-over-n" markets occur in situations where the ability to compete is determined mainly by intangible assets. Professor Hausman argues that the FCC licences constitute the essential intangible asset that enables radio stations to compete. He then further supports his conclusion with a table of listener share changes over a one, two, and three year period that shows the share of listeners accruing to individual radio stations to be quite volatile over time. I have reached the following conclusions regarding Professor Hausman's conclusion.

- I disagree with Professor Hausman's conclusion that radio firms should be assigned equal market shares. I do not believe that radio firms are good candidates for being considered as one-over-n markets, and the table of listener share changes he reports misrepresents competition among radio firms.
- Gregrory Werden argues that "Candidates for the assignment of 1/n shares include markets for technology or innovation and Schumpeterian industries, in which competition occurs largely through the introduction of new products or technologies and competition is apt to be more "for the market" than "in the market."[5] Radio stations do not easily fit this characterization. As Professor Hausman argues, FCC licences are essential assets for terrestrial radio stations, but not all FCC licences are alike. The power of the broadcast signal and the location of the broadcast tower differentiate the licences. In addition, radio competition does not only occur through the addition of new products. Stations do switch formats, and new formats do get introduced, but stations also retain the same formats for

---

[4]*Assessing Market Shares*, Antitrust Law Journal, (2002), V. 70, 67-104.

[5]Ibid, p. 86.

years. Examining radio station formats from 10 markets for the period 1988-1998, Charles Romeo and Andrew Dick[6] found that 16.6 percent of stations changed their format category, while 13.3 percent more made within category format changes, and 70 percent of stations made no format changes.[7] Finally, competition is likely to be "in the market" as opposed to "for the market." Radio has the characteristics of a two-sided market. Stations produce listeners which they then sell to advertisers. To reach listeners in certain demographics, advertisers may place ads on multiple stations, some within the same format, others in very different formats thereby providing room for the multiple stations.

- Changes in station outcomes are attributable to both the power of their licences and the programming choices, and it is exactly in this sense that I argue that Professor Hausman's table on listening share volatility misrepresents competition in the market. The three year changes in the table are meant to suggest that a station that begins with a high listening share is almost equally likely to end up with either a high or low share at the end of three years, and likewise for a station that begins with low share. Rather, tower location and the power of a station's FCC licence are likely to keep stations within certain listening share ranges. High power stations that are centrally located in a market are more likely to remain among the top-tier in listening share, while low power stations or stations that only broadcast to a portion of the market are more likely to remain among the lower-tier. Moreover, even within low-tier and top-tier stations, changes in station outcomes are not

[6] *The Effect of Format Changes and Ownership Consolidation on Radio Station Outcomes*, Review of Industrial Organization, (2005), V. 27, 351-386.

[7] An example of a format category change would be a change from Country to Rock, while an example of a within category change would be Country to Classic Country.

entirely random. They are driven by observable factors. For example, Romeo and Dick[8] explained nearly 53 percent of listening sharing variation through regression on observable factors readily available from Duncan's and BIA.

- Even staying within the context of culling information on market share volatility from simple share change tables, there are two additional sets of results that Professor Hausman could produce that would provide alternative views of the degree of volatility. First, I would argue for creating a second table with entrants and exits removed, as these are likely to be a substantial portion of the stations with the largest listening share growth and decline. Second, a table of listener share rank correlations over a three year period may produce a substantially different picture of the degree of volatility over this three year period.

---

[8]Ibid.

STONY BROOK
STATE UNIVERSITY OF NEW YORK

Department of Political Science
Stony Brook University
Stony Brook, NY 11794-4392
Phone: (631) 632-9761
Fax: (631) 632-4116
Email: Stanley.Feldman@sunysb.edu

October 1, 2007

To: Jonathan Levy
From: Stanley Feldman
Re: Review of *How Journalists See Journalist in 2004* and *Media Professionals and Their Industry: A Survey of Workers*

As you requested, I have read all of the material that was included with two reports: *How Journalists See Journalist in 2004*, by the Pew Research Center and *Media Professionals and Their Industry: A Survey of Workers*, by Lauer Research, Inc. These two studies cover very similar issues although their methodologies vary somewhat. The Lauer Research, Inc study surveyed members of the media industry who are members of one of four unions: AFTRA, NABET, The Newspaper Guild, and the Writers Guild of America. The Pew Research Center Study interviewed people who were systematically sampled from people working in the news industry in television, newspaper, wire services, magazines, and news services. It is difficult to know how similar these two samples are in practice since there is not enough information provided in the Lauer Research report to make comparisons to the composition of the Pew Study.

In addition to detailed information about the composition of the Lauer Research report there are two other critical omissions that make it very difficult to evaluate the conclusions of this study. First, the appendix with the figures and charts was not included with the report. It is therefore not possible to see much of the data presentation that is referred to in the text. And as the questionnaire that was included with the report does not contain any frequencies for the questions there is a great deal of missing information. It is therefore impossible to check the figures that are provided in the text against any data from the survey. Second, the technical details of the study do not give any indication of the response or cooperation rate for the survey. The report simply says that "interviewers made up to three call back attempts per household" to reach the members of the sample. They could have had an 80% cooperation rate or a 20% cooperation rate. Without this information, or other information that compares the characteristics of the sample with known characteristics of the union members, it is impossible to know how good the sample is – whether it is really representative of the population of union members. In general, three call backs is *not* enough to insure a good response rate for a telephone sample and I fear that the response rate for the study is probably too low.

I also feel that the Lauer Research report sometimes tried to draw conclusions from relatively week data. Small differences in percentages were described as being large, conclusions about potential changes in attitudes were drawn from data collected at one time point, and there were far more questions that asked about problems in the media industry than one that asked about strengths. With all of these limitations it is difficult to know how much faith to put in the conclusions drawn in this study.

The Pew Research Center study (*How Journalists See Journalists in 2004*) is much more detailed than the Lauer Research report and provides all of the elements missing from the Lauer report. The technical report is detailed and describes exactly how the population was stratified and how specific respondents were selected. Repeated efforts, by mail and telephone, were used to maximize the response and interviews were completed with 67% of those in the sampling frame – a very good result. It is possible to confident that the sample accurately reflects the views of the population of news media professionals that it was designed to study.

The Pew Research study also has two other major strengths. The questionnaire that was used was much better balanced than in the Lauer Research study. It was more detailed and, more importantly, much better balanced. As a result, it is possible to get a much clearer picture of the ways in which news media professionals see the news industry. In order to understand the magnitude of the negative comments it is often necessary to compare them to positive comments as is done in this study. The Pew Research report also benefits from having some earlier data (surveys of news media professionals in 1995 and 1999) to compare the 2004 data with. As a result, at key points it is possible to see actually change over a 10 year period. This is a major improvement over the common practice (used in the Lauer Research study) of asking people whether a situation has gotten better or worse. It is entire possible that a majority of respondents could say that "things" are getting worse at several points in time without actual views really changing. Having some over time comparison increases confidence in inferences about change.

I found the Pew Research report to be careful and appropriately detailed. Conclusions were clearly backed up with data and were qualified where necessary. The discussion was even handed throughout, balancing positive and negative assessments and, where appropriate, showing the diversity of views across the news media industry. The discussion of ideology – a generally contentious topic – was particularly well done. This is a solid study and the conclusions are well supported by the survey data.

PHOENIX CENTER FOR ADVANCED LEGAL
& ECONOMIC PUBLIC POLICY STUDIES
5335 Wisconsin Avenue, NW, Suite 440
Washington, D.C. 20015
Tel: (+1) (202) 274-0235 • Fax: (+1) (202) 318-4909 • DIRECT DIAL: +1 (205) 909-3709
www.phoenix-center.org

George S. Ford, Chief Economist

3 September 20071 September 2007

Jonathan Levy
Deputy Chief Economist
Federal Communications Commission
445 12th Street S.W.
Washington, D.C. 20554

RE: Peer Review of REVIEW OF THE RADIO INDUSTRY, 2003 by George Williams, Federal Communications Commission

Dear Jonathan:

At your request, I have reviewed the study entitled Review of the Radio Industry, 2003, by Commission Senior Economist George Williams. Per your instructions, I have considered the following: (1) whether the methodology and assumptions employed are reasonable and technically correct; (2) whether the methodology and assumptions are consistent with accepted economic theory and econometric practices; (3) whether the data used are reasonable and of sufficient quality for purposes of the analysis; and (4) whether the conclusions, if any, follow from the analysis. Also per your instructions, I will not "provide advice on policy," but limit my discussion to the four listed standards above. I am aware that this review is not anonymous. To my knowledge, I have no potential conflicts of interest in this proceeding or on these issues more generally.

The Review of the Radio Industry, 2003, is an 82-page document excluding a title page. There are 13 tables and charts in the 29-page body of the document. There are 6 Appendices.

This study is primarily a collection of statistics on the radio broadcast industry. No new theoretical or empirical techniques are proposed, presented or employed. The discussion of the descriptive statistics relies on established techniques and theoretical concepts. For example, the study's discussion of market concentration makes use of the concentration ratio (CR1, CR2, and CR4), which is a widely accepted and informative measure of market or industry concentration. The financial ratios used in Section 4 (Radio Industry Financial Performance) are also established indicators of financial performance from both a practical and theoretical perspective. As such, their use in a study of this type is reasonable. Further, the interpretation of the trends in these financial indicators is consistent with standard professional practice. While others may have different interpretations of the trends, those used in this study are sensible and consistent with professional standards. It appears that sufficient detail and discussion on these financial ratios is provided so that the underlying data could be reproduced by other researchers.

Perhaps the most difficult aspect of radio broadcasting to measure is Format Diversity. The count of formats statistic used in this study is a simple yet plausible measure of Format Diversity.

Much of the data is based on Arbitron defined radio markets, which is consistent with FCC policy on market definition for radio broadcasting. Data used for this study are provided by BIA, Compustat, Arbitron, and Service Quality Analytics Data (SQAD). All of these data sources are generally viewed as reliable and their use for this study is reasonable. Some relevant details and limitations of these data sources are discussed in the study, particularly with respect to the BIA data on ownership.

As for specifics, the statement on Page 16 that the "market to book ratio is a good proxy for a firms 'q' ratio" needs some qualification. The q-ratio is the ratio of market to replacement value, and book value need not represent replacement value. A cite to a study or two that use the market-to-book ratio as a proxy for the q-ratio seems adequate. Also, there are two issues of document format. First, there appears to be a formatting problem with the footnotes, with some having a return between notes while others do not. Second, the study has no Conclusion.

Overall, it is my opinion that: (1) the methodology and assumptions employed are reasonable and technically correct; (2) the methodology and assumptions are consistent with accepted economic theory and econometric practices; (3) the data used are reasonable and of sufficient quality for purposes of the analysis; and (4) the conclusions follow from the analysis. The study is well written, well documented and conveys useful information to both researchers and policymakers.

Sincerely,

George S. Ford
Chief Economist

Peer Review of "Relaxation of Media Ownership Limits and Concentration of Media Markets Undermines Minority Ownership," by S.Derek Turner and Mark Cooper (Study 13)

By David Genesove, Professor of Economics

Dr. Walter Hesselbach Chair in Economics

Hebrew University of Jerusalem

August 29, 2008

The first part of this study presents a count of stations that are owned by minorities and a further breakdown according to the race/ethnicity of the owners, for each of three years: 1998, 2000 and 2006. The counts are based on FCC Form 323 Filing, NTIA and Free Press Research. I have no way of assessing the quality of the assignment of stations to minority or more detailed status and will simply assume that it is done correctly.

The figures show clearly that the rate of minority ownership was no different in 2006 than it was in 1998. The component parts of the rate attributable to the different race/ethnicities are not stable, however. The rate of African-American ownership fell substantially (although not statistically significantly) [1] over that period. The authors stress the fall in African-American ownership, while ignoring the increase in the Hispanic, American Indian/AK Native and Asian ownership rates.

The authors are careful not to explicitly infer anything about the relationship between concentration policy and minority ownership from these figures, but they do allow themselves the comment that "there has been no improvement" in minority ownership "despite" the increase of television stations by about 12 percent. No explanation is given as to why an increase in the total number of stations would normally have led to an increase in the rate of minority ownership, but the implication is that the relaxed rules on concentration have offset what

[1] There is no attempt to test for the significance of the fall in the African-American ownership rate. Given that these ownership rates are so small, one would expect substantial volatility in them. Indeed, the p-value for the Fisher exact test for no change across 1998 and 2006 is .17, indicating no significant change, using typical accepted threshold values.

otherwise would have been an increase in minority ownership. Inferring a negative effect of policy changes from a concurrent negative change in some outcome without a control group is always problematic; it is additionally so here when one of the policy change (the increase in the national cap from 25% to 35%) occurred in 1996, before the first year analyzed here, and when the change of the outcome of interest is not actually observed to be negative but is surmised to be so on the basis of some unspecified theoretical mechanism.

The authors go on to compare the average number of stations owned by minority versus non-minority owners,[2] and male versus female owners. Minority and female owners are shown to own fewer stations. The authors argue that consequently, minority and female owners are thus "more likely to better serve their local communities than stations controlled by large group owners", on the basis of an FCC study that showed that locally owner stations broadcast more local news. This argument sees minority/female ownership as a *mechanism* to ensure local ownership and so can not serve as part of a critique of relaxed concentration rules that is based on the effect of those rules on minority/female ownership. Furthermore, the authors fail to show that effecting local ownership through minority/female ownership is preferable to effecting it through direct policy rules on local ownership.

The authors also state that "minority and female station owners are more likely … to feel the negative effects of increased consolidation." This may be the case, but the authors provide no empirical evidence of it. No theoretical argument is given either, and, indeed, in standard models of competition, in which

[2] The latter would appear to include corporate owners.

category broadcasting markets admittedly do not fall, non-merging firms typically benefit from the consolidation of other firms.[3]

The second part of the study identifies those sales of minority-owned stations that would not have been permitted under the old concentration rules. The implication is that had the policy changes not taken place, these stations would not have been sold. However, this is not necessary so: the minority owned stations might still have been sold, but simply to other owners whose acquisitions of the stations would not have violated the previous rules. Put another way, the changes in the rules may not have affected which stations were sold, only to which buyers they were sold.[4]

The last part of the authors' analysis is a statistical analysis of the relationship between the incidence of a minority owner in a market and the degree of market concentration. The results show that across markets, and given the market rank and the fraction of the population that is minority, the incidence of at least one minority owner is associated with lower concentrated.

The authors claim that this (partial) correlation between the presence of a minority-owner and low concentration suggests "that minority-owners thrive in more competitive markets, regardless of market or station characteristics". But this inference can not be made.

---

[3] Salant, Switzer, and Reynolds, "Losses from Horizontal Merger: The Effects of an Exogenous Change in Industry Structure on Cournot-Nash Equilibrium", *Quarterly Journal of Economics*, May 1983, 98:185-99, and Deneckere and Davidson, "Incentives to Form Coalitions with Bertrand Competition", *The RAND Journal of Economics*, 16(4), 1985, 473-486.

[4] A more convincing argument, although would still not be definitive, would have been to show that the fraction of sales of non-minority owned stations that would not have been permitted under the old rules was significantly lower than the fraction of sales of minority owned station that would not have been permitted.

The results are equally well interpreted as saying that where there is a small owner, the market is likely to be less concentrated. By definition, allocating a station (whether an additional one, or one previously owned by a multi-station firm) to a single owner will decrease the concentration rate. The authors have shown us that minority owners hold fewer stations overall; it seems reasonable to assume that they own fewer stations within any given market as well. Thus minority owner may simply be proxying for single owner here; and such an association is simply a definitional artifact.

There are two additional problems with the statistical analysis in this part. First, concentration should be a function of market size, not market rank: the larger the market, the more stations that can operate profitably in it; it will not matter how many other markets there are that are bigger than the market of interest. If there are regulatory constraints that are defined according to market rank, then market rate will be a determinant of concentration, but in addition, not in place of, market size.

Second, it is well known that linear probability, probit and logit estimates are typically (corrected for scale; Amemiya, *Journal of Economic Literature*, 19(4), pp. 1483-1536.); it is not much of a robustness test to estimate the relationships with all three functional forms.

From: TPMG4817@aol.com [mailto:TPMG4817@aol.com]
Sent: Wednesday, September 19, 2007 1:50 PM
To: Jonathan Levy
Subject: Re: Reviseon of peer Review #2

20 September 2007

Peer Review of FCC
Study 14

A CASE STUDY OF WHY LOCAL REPORTING MATTERS: PHOTOJOURNALISM FRAMING OF THE RESPONSE TO HURRICANE KATRINA IN LOCAL AND NATIONAL NEWSPAPERS

Is this study reasonable and objective? Yes. This is a fine study if one accepts the assumptions that the authors posit. That is they compare newspaper coverage as done by New Orleans newspapers and then out of town newspapers -- in a vacuum. But accepting the assumptions, they demonstrate that more extensive, local coverage of an catastrophe by accepted journalistic standards seems to be done more completely on a local level than are national summaries. But I am not sure how this raises concerns about the combination of a business which might both own a newspaper and a TV station.

Is it consistent with standard economic methodology? Yes. Newspaper economics means profit making and local coverage is less costly and draws more buyers and so means more profits.

Are the data relevant to the question at hand? Yes -- as far as they go. The authors point out that Internet access changes the basic assumptions. The authors thus offer no data as to how many people used the Internet to access the newspaper coverage by the New Orleans newspapers than from outside the city. In a natural disaster this might be considerable.

Are the data sufficient quality? Yes -- as far as the limitations of the data can take them. The collection of the data relies on coding of the photo images. This adds a subjectivity to the analysis.

Do any of the conclusions reached follow from the analysis, the basic questions at issue?
It is too harsh -- to use the author's language -- to say that non-New Orleans newspapers simplified, exaggerated and distorted (which implies intent) the image of the Katrina disaster. All other things being equal, people would have been better off to have read the coverage of the events from both a local and national view points of view.

But so long as the Internet is open, affordable, and is part of many newspaper's operations, then they can.

That race matters in the USA hardly needs to be demonstrated. But reading multiple sources of information with an open mind has always been the answer and the Internet makes this easy for those who can afford connection and who have the necessary time.

Simplifying is not bad, just a matter of allocating that most precious of goods: time.

Winning Pulitzer Prizes should be ignored as a criterion.

Douglas Gomery
Resident Scholar
Library of American Broadcasting
University of Maryland

From: TPMG4817@aol.com [mailto:TPMG4817@aol.com]
Sent: Wednesday, September 19, 2007 12:11 PM
To: Jonathan Levy
Subject: Draft 2 from Gomery

20 SEPTEMBER 2007

NETWORK BROWNOUT REPORT, NAHJ Staff

Is this study reasonable and objective?

The National Association of Hispanic Journalists has been studying coverage of Latinos on the network evening news for the past dozen years in an effort to argue the lack of coverage of this important sub-group in U.S. culture.

Sadly they study only the NBC, CBS and ABC network evening news. With around the clock news outlets -- particularly the Internet for many -- radio for all -- the nightly news has shrunk to an audience of basically persons over age 50

Not surprisingly stories about Latinos remain absent on theses three nightly network news summaries.

They also object that those stories that make it to the air are -- as they see and hear and analyze them -- are often stereotypical. Although the U.S. Latino community has grown significantly in the dozen years of this report, the amount of coverage devoted to Latinos has not reflected this growth.

Is it consistent with standard economic methodology? No. This is social analysis, not economic analysis.

Are the data relevant to the question at hand? Yes. But see below.

Are the data sufficient quality? They take this data because it is the only source for analysis that is easily accessible. As in prior studies, Latino-related stories were identified by searching Vanderbilt University's Television News Archive. Other networks, such as Fox and MSNBC, are not fully archived and thus were not included.

Do any of the conclusions reached follow from the analysis, the basic questions at issue?

Yes. They make their point that on the network evening news that the vast majority of immigration stories were not told from the Latino perspective. NAHJ strongly believes that increasing the number of Latino journalists and managers will improve news coverage of the Latino community.

The goals of this report are to bring greater awareness to how Latinos are being portrayed on national news programs and to urge the networks to increase and improve their coverage of the Latino community. The news media's poor media coverage of Latinos and people of color is historic. In 1947, the Commission on Freedom of the Press, also known as the Hutchins Commission, outlined the responsibilities of the news media in a democratic society. Among its five major recommendations, the commission stated that the press should project a representative picture of the constituent groups in the society. Twenty years later, President Lyndon Johnson appointed the National Advisory Commission Civil Disorders, known as the Kerner Commission, to examine the causes of the riots that erupted across the country in the late 1960s. Among its major findings, the Kerner Commission concluded that the then media inaccurate portrayals and misrepresentations of the black community contributed to the racial division Both the Hutchins and Kerner Commissions urged the nation's news media to improve their coverage of minority communities.

Since then, news coverage of people of color has increased.

Sadly this is an analysis with a conclusion already set in place.

Douglas Gomery
Resident Scholar
Library of American Broadcasting
University of Maryland